	Years.	Rain. In.	Sums. In.
Max.	1859	56·22	187·95
	1860	57·91	
	1861	73·82	
Min.	1866	59·68	174·21
	1867	69·93	
	1868	44·60	
Max.	1871	41·46	154·85
	1872	48·39	
	1873	65·00	

Grouping the results we obtain :—

Rain in Max. Years.	Rain in Min. Years.
184·42	163·67
187·95	186·70
154·85	174·21
527·22	524·58

; showing an excess of 2·64 in. on the maximum side.

The quinquennial periods, as far as they admit of comparison, give also an excess in favour of the maxima years.

The heavy falls in 1844 and 1855, and the comparatively small fall in 1872, are apparently opposed to the theory; but it should be borne in mind that rainfall is greatly affected by local causes, and that to reveal the effects of a weaker but more general cause we must, as far as possible, eliminate chance, by comparing the total falls in maxima and minima periods. Tried by this preliminary test, the experience of Barbados can scarcely be said to be opposed to the theory.

My main object, however, is to draw attention to some discordances between Mr. Rawson's figures and those given by Mr. Symons in NATURE (vol. vii. p. 143); for until this disagreement be explained, there will be considerable uncertainty respecting the rainfall of Barbados. The following table will show where the two statements are at variance :—

	Years.	Rain. (Mr. Symons.)		Rain. (Mr. Rawson.)	
Min.	1843	45·3	163·7	45·31	163·79
	1844	74·5		74·45	
	1845	43·9		43·91	
Max.	1847	42·5	158·3	48·10	164·64
	1848	62·8		63·77	
	1849	53·0		52·77	
Min.	1855	73·5	170·7	77·31	186·70
	1856	46·4		48·49	
	1857	50·8		60·90	
Max.	1859	55·1	186·6	56·22	187·95
	1860	60·4		57·91	
	1861	71·1		73·82	

The greatest differences are in 1847, 1855, and 1857, and amount (for these three years alone) to 19·4 in.

It is worthy of remark that both statements show an excess on the side of the maxima years; Mr. Rawson's of 2·2 in., and Mr. Symons's of 10·5 in. But how did such great differences arise.

A remark made by Mr. Rawson may explain the matter. He says "the average of the island for twenty-five years, from 1847 to 1871, is 57·74 inches, based upon the mean of three stations in 1843, and increasing to 141 in 1871." Now it would be useful to know how the mean yearly rainfalls were determined. Is the fall given for 1844 (74·45 inches) a mean of the falls at three stations, and the fall for 1872 (48·39 inches) a mean of the falls at 141 stations? If so, and if the other yearly means were similarly obtained, Mr. Symons may not have taken the same number of stations as Mr. Rawson. Yearly means thus determined would not of course be comparable, for even in a small island the rainfall varies greatly according to locality. The rainfall in maxima and minima sunspot years cannot be fairly compared except by taking the same number of gauges and the same stations; and it is desirable that the falls in the intervening years should be given.

"Assuming that sunspots affect all parts of the globe *equally*, and that periodicity prevails in all *alike*," Mr. Rawson, with the above experience of Barbados before him, is "led to the conclusion that it was 'chance alone' that led to the coincidences noticed by Mr. Symons." Now the theory makes neither of these assumptions. It assumes that there is a sunspot periodicity; that this periodicity implies a secular variation of solar heat and radiation; that, therefore, there is a corresponding periodicity of temperature, wind, and rain on our earth; but that, from various counteracting causes, the observations at some stations will not show a periodicity, while those at a large majority of stations, and a mean of all the observations, will do so. In short, with respect to rain, the theory assumes that the annual fall over the globe is subject to a variation, corresponding with the sunspot variation, but that from disturbing influences, local exceptions must be expected, Granting, therefore, that the rainfall of Barbados is opposed to the theory, I do not think it follows that the favourable experience of the British Isles must be owing to chance alone; for that experience is what theory leads us to expect, and it is much more extensive both as to time and space than the experience of Barbados. If England and Barbados were the whole globe, the theory would be well-nigh proved, as far as observation goes; for, according to Mr. Symons's Table 1, there was not, from 1815 to 1864, a single exception to the rule that more rain falls in the maxima years; and if we take the aggregate falls for England and Barbados from 1843 to 1873, it will be found that there was a large excess on the maximum side.

I have now examined 93 rainfall tables from various parts of the globe. They are all I have as yet been able to procure, and they have been published *in extenso*, so that the evidence they afford may be scrutinised. That evidence is such that if no rain at all had fallen at Barbados in the nine principal maxima years since 1843, and the rainfall in the nine minima years were to be put in the other scale of the balance, there would still be a large surplus in favour of the theory. Up to the present time the more numerous the observations, the stronger the evidence. Still I shall be prepared to abandon the theory whenever a preponderance of undoubted facts may be brought against it. But I see no prospect of this, for the rainfalls of England, Scotland, the Continent of Europe, India, Africa, America, and Australia, as far as they have yet been examined, sustain the theory.

C. MELDRUM

Mauritius, Sept. 15

Dr. Sanderson's Experiments and Archebiosis

DR. SANDERSON has strangely misunderstood the wording of my letter which appeared in NATURE on the 9th inst. Any one may see that I did not challenge him to "deal" with my main proposition "that Bacteria are capable of arising in fluids independently of living reproductive or germinal particles." That position was merely alluded to by me in order to show the relevancy of the question which I asked Dr. Sanderson : and the question itself was—"Whether he still believes that Bacteria are killed by a temperature of 100° C. in fluids; and if not upon what grounds he has changed his opinion?"

Whilst tacitly declining to answer this question, Dr. Sanderson now says, "I hope that Dr. Bastian will allow me to decline to enter on the general question." But it is precisely because Dr. Sanderson has distinctly expressed himself upon the general question both at the late meeting of the British Association and in your columns (NATURE, vol. viii. p. 181), that I feel he may, both from a moral and from a scientific point of view, be called upon to reply to the question above quoted.

The need that Dr. Sanderson should express the grounds of his opinion concerning the death point of Bacteria in heated fluids is further shown by Mr. Ray Lankester's communication in last week's NATURE, in which he says, "Dr. Sanderson does not believe that there is a definite relation between the precise temperature to which the infusion is exposed and the destruction of Bacterian contamination." Now if this is really Dr. Sanderson's present opinion, it may not inappropriately be asked whether it is an opinion based upon definite evidence or whether it is a mere surmise? I say the question is not inappropriate because, as Dr. Sanderson will recollect, I have heard from his own lips, since his return from Bradford, that he has made no definite observations upon the subject, and that he is quite unprepared to question the truth of the experimental evidence which I have recently brought forward (Proceed. of Royal Society, Nos. 143 and 145) showing that Bacteria are killed in fluids which have been raised for five minutes to a temperature of 60° C. (140° F.)*

Dr. Sanderson previously supposed that Bacteria were incapable of appearing and rapidly multiplying in certain fluids

---

* I should have hesitated about referring to what has passed in conversations between Dr. Sanderson and myself, if he had not set the example both in your columns (NATURE, vol. viii. p. 181) and in a discussion at one of the meetings of the Royal Society.

© 1873 Nature Publishing Group

raised to 100° C. and subsequently protected from contamination. He has been convinced that his supposition on this subject was erroneous. And since this period, whilst I have been careful to undertake fresh researches concerning the death point of Bacteria, he has been content to rest in the stage of mere supposition on this most important point, and is now, as it appears, quite unprepared to question the truth of my assertion that Bacteria are killed at 60° C. It is right that the public should know this, and I only regret that Dr. Sanderson himself cannot be induced to inform them as to the real extent of his knowledge upon this part of the subject.

H. Charlton Bastian

University College, Oct. 20

Foreign Orders

The acceptance and refusal of foreign orders by British subjects has hitherto been universally misunderstood. The existence of the Queen's Regulations, which you have reprinted in your columns (vol. viii. p. 481), prohibiting the receipt of these orders without special permission, must, after the discussion which took place in the House of Commons during last session, surprise many of your readers, who will naturally ask why regulations so stringent and so habitually disregarded, have been either kept entirely private in the Foreign Office, or, if published, have never been followed up. As it is, I will venture to say that not one out of some hundreds who have received foreign orders are aware of the prohibition or have any obvious means of becoming aware of it. Announcements of the presentation to British subjects (and it is assumed acceptance of by them) of such orders habitually appear in the most conspicuous type of the most widely circulated papers, but never a hint on the part of the Foreign Office that the recipients are violating Her Majesty's rules, as drawn up by itself and signed by the Secretary of State for Foreign Affairs.

Such being the case, it is somewhat singular that the Foreign Office should issue regulations approved by Her Majesty, forbidding British subjects to accept or to wear foreign orders and their decorations, except in the very rare cases in which Her Majesty's permission is obtainable, and yet take no steps through its agents at foreign courts to instruct the habitual givers that Her Majesty not only disapproves of their action, but requires of her subjects to tell them so in the most ungracious of all ways, namely by refusing to accept their favours, and returning the tokens thereof.

Surely if the prohibition to accept is wise and good (and I am the last person to doubt Her Majesty's wisdom) the obvious course for the Foreign Office to pursue is to inform all foreign Sovereigns of the fact, and instruct British subjects to transmit any orders that they may receive or have received to the Foreign Office to be returned to the sovereign who sent them, if the services of the recipient are not of such a nature as to enable him to obtain permission to accept them.

Into the merits of the prohibition I am not disposed to enter at much length. That foreign orders are comparatively valueless in themselves is generally admitted; and it is well understood that not a few are to be had for the asking by men of real or supposed eminence, and others by solicitation from men of no eminence at all, or of doubtful eminence. It would surprise your readers to know how many of these orders there are in the possession of their countrymen, whose habitual disregard of such honours leads them in most cases to toss them into a drawer and say nothing about it to any one but their wives, who think they would suit their necks better than their husbands' long-tailed coats.

Some few (very few) no doubt have a definite scientific or literary value; but so long as the British public are entirely ignorant of this value, they will be held in no higher estimation than the others, nor do I see any way by which the value of a foreign order could be made known and recognised, or by which the title of the recipient to wear it could be appraised

I believe that it is to the rarity of British orders that any desire to obtain foreign ones is mainly due. Had we more, or none, their value would diminish or expire; as, however, I am not prepared to propose either the restriction or multiplication of British orders, a third alternative might be suggested to the Foreign Office, and that is the command to wear them if accepted; which would result in a display in our *soirées* and assemblies of which men of eminence would be heartily ashamed, and lead to a petition for relief, that would be followed by an abandonment of the practice of giving by the powers that be.

D.C.L.

Mr. Forbes on Mr. Mallet's Theory of Volcanic Eruption

I do not intend to depart from my purpose, as stated in my last (Nature, vol. viii. p. 485), to have done with further controversy. I must, however, beg your permission to correct a statement as to a matter of fact which constitutes the prominent feature of Mr. D. Forbes' letter on the above, and which is published in the last number of Nature.

Mr. Forbes says, and begs your readers to remember that his remarks [namely, in his original review of my translation of "Palmieri"] were altogether directed to the assertions contained in my introductory sketch, and not *comments upon my theory of volcanic energy*—of which Mr. Forbes now says we, viz., he and your readers, as yet know little or nothing. That is to say, nothing beyond what is given in the abstract in the Proceedings of the Royal Society and in my Introduction to Palmieri.

Mr. Forbes' review (Nature, vol. vii. p. 259) which called forth this correspondence, was no doubt confined to my translation of, and introduction to, "Palmieri's Vesuvius," &c. But in that same introduction was contained a sketch of my theory of volcanic energy—upon which Mr. Forbes deemed himself warranted to make his sweeping condemnation—that it was not probable that this hypothesis will receive the adhesion of either chemist, mineralogist, or geologist.

If this were not a comment upon my theory of volcanic energy I know not what a comment means.

My complaint has been that it was a comment condemnatory —based on erroneous as well as inapplicable premises—and made at a time when, as Mr. Forbes himself in his last admits, he knew very little about that theory, as fully expounded in my paper in the Phil. Trans.

Robert Mallet

Oct. 28

Settle-Cave Report

I have just read with considerable astonishment Mr. Tiddeman's letter (Nature, October 23) relating to an abstract which I never saw till to-day, and for which, therefore, I am not responsible. The whole question of the antiquity of cave-deposits as well as thatof those in the Victoria Cave, in particular is treated in my work on "Cave-Hunting," shortly to be published, and therefore I see no reason for entering into any argument based on the distribution of the Pleistocene Mammalia, or to depart from my rule of not entering into a controversial correspondence.

W. Boyd Dawkins

Owens College, Manchester, Oct. 24

The Oxford Science Fellowships

I write to confirm Prof. Clifton's letter (in the last number of Nature) respecting Mr. Perry and Oxford Science Fellowships. Nothing, it seems to me, can be more conclusive than the way in which Mr. Perry's letter has been answered. Any remark further of mine on this point would be superfluous.

I will only say that, in the practical part of the examination, no subject could have been chosen better fitted for giving perfectly fair play to all concerned. If it were possible to imagine that any advantage was given, it was, by the choice of the subject, given to those who were unacquainted with the University laboratory.

In conclusion—far from being looked on as an unwelcome intruder, I met with from all, whether candidates or examiners, the most generous courtesy and kindness.

Cambridge, Oct. 24

The Cambridge B.A.

Professor Clifton cannot have considered what a great mistake I have been the victim of, or he would not in his hastily written attempt to defend the general science arrangements at Oxford, have forced me to the following explanation. He knows that I stated my case fairly, and he might surely have given credit for this whilst letting us have the benefit of his later information.

1. I have not at hand a copy of my letter to the Warden. I am quite sure that I told him I was a graduate of the Queen's University in Ireland. The Warden simply directed me to the short notice in the *Times* (afterwards given in your columns), said that the election would not be limited to graduates of Oxford, and would altogether depend on the results of the examination held at Merton on Oct. 7. I thought this letter perfectly satisfactory

© 1873 Nature Publishing Group